

consideration of the objective facts which Mr. Bateson wholly shirks.

I now come to the other point. I put colour change entirely aside for reasons which seem valid to me, and which I may take another opportunity of explaining. Apart from these the cultivated *Cineraria* exhibits no variation from the feral form which may not be described as dimensional. While the foliage has remained approximately constant, the loose corymbose habit has been contracted into a tight corymb, and the heads of florets have been enormously enlarged. While the feral form stands about five feet high, the cultivated one is about eighteen inches. I am disposed to restrict the term "sporting" to a definite morphological change such as is exhibited in the flowers of the garden *Chrysanthemum*, and recently in the occurrence of an "ivy-leaved" form of the Chinese Primrose. But except a race of so-called double *Cinerarias*, which did not take the public fancy, the history of the garden *Cineraria* does not present, as far as I know, any trace of a real morphological change. If I might venture to use a mathematical analogy, I should say that the form of the *Cineraria*-function has remained unaltered.

Now the object of these dimensional changes has been to make the plant worked upon handy and convenient for decorative purposes. Those points which were unessential for this purpose have been unconsciously neglected, and their stability has not been affected. But I do not doubt that if it had been otherwise the *Cineraria* might have been brought by this time to any configuration which the cultivators fancied.

As far as I can make out, the transformation of the *Cineraria* has taken about sixty years to effect. Mr. Bateson will not complain if I quote a few words from one of his own authorities of about that date:—"One species especially merits cultivation, viz. *C. cruenta*. This may be regarded as the parent of many of those beautiful varieties which are so successfully cultivated by Messrs. Henderson." Now my memory of the cultivated *Cineraria* goes back some thirty years. I can remember when it was a rather lanky plant, about half the height of the feral form, with a somewhat lax inflorescence and far smaller flower-heads than are now to be seen. The present fashionable *Cinerarias*, with a very condensed inflorescence and very large flower-heads, only date back some ten or twelve years.

I see, therefore, no reason for abandoning my assertion that the evolution of the modern *Cineraria* has been slow and gradual, and not *per saltum*, and this is in accord with general horticultural experience. As soon as a new plant is introduced, every one wants to get a form with bigger flowers or floral structures than anybody else. There is only one secure path to this result, and that is by taking advantage of seminal variation and selecting the minutest trace of change in the desired direction. By patiently and continuously repeating the operation, almost any desired result can be obtained. The horticultural gambler may hope to reach it by a "sport," but he will not. *Anthurium scherzerianum* is a good illustration. Introduced in 1862, it was little more than a curiosity; now its enormous and brilliant spathes are a conspicuous object at every flower-show. This has simply been accomplished by progressive selection working on seminal variation.

Mr. Bateson has now the coolness to say that "the hybrid origin of cultivated *Cinerarias* is of subordinate interest." All I can say is that in that case it is a pity that he wasted three columns of NATURE with a discussion of the subject. I should have thought myself that it was a matter of very considerable importance indeed to be able to form an approximate idea of the amount of change in a given time in an unmixed species, and so obtain some measure of the possible rate of evolution, at least in regard to dimensional characters.

For my part, I think that in the study of evolution we have had enough and to spare of facile theorising. I infinitely prefer the sober method of Prof. Weldon, even if it should run counter to my own prepossessions, to the barren dialectic of Mr. Bateson.

W. T. THISELTON-DYER.

Royal Gardens, Kew, May 13.

Some Bibliographical Discoveries in Terrestrial Magnetism.

I HAVE recently made some interesting discoveries pertaining to the history of Halley's famous chart of the Lines of Equal Magnetic Variation (Declination), to which renewed attention is just

now being called by Prof. Hellmann's admirable facsimile reproduction of the earliest geomagnetic charts.¹

The first reproduction in facsimile of Halley's chart was undertaken by G. B. Airy, and published in "Greenwich Observations" for 1869. Airy was led to do this by reason of the fact that he could find no geomagnetician of his time who had ever seen Halley's chart. After diligent inquiry among academies and libraries at home and abroad, it was found that the British Museum possessed a copy, and it was believed, the *only* copy extant. Since then, Prof. Hellmann has succeeded in tracing two other copies, one at Hamburg (Stadt Bibliothek) and one at Paris (Bibliothèque Nationale), and has also, since the publication of his book (as he has just informed me), come into possession of a copy himself.

I have personally examined the Hamburg and Paris copies, and, during a brief stay in London in March, also the copy in the British Museum used by Airy. I have found, moreover, in the British Museum, three other Halley charts and two Dutch reprints. By a careful and critical study of these various copies, some new light is thrown upon the publication of Halley's chart. To make this apparent, some wearisome details with regard to the various copies will be necessary. I will begin with the British Museum copies.

Catalogue No. 974 (5).—"A new and correct Sea-chart of the Whole World, showing the Variations of the Compass as they were found in the Year 1700, by Edmund Halley." Date (according to the Catalogue), 1701.

The above is the English title of the chart referred to at times by the Latin title, "Tabula Nautica," &c. This copy appears to be the one used by Airy in his facsimile reproduction of the Halley chart published in "Greenwich Observations" for 1869, which in turn has been used for Prof. Hellmann's reproduction. There is no date on the chart, nor the name of the publishing firm. The date 1701, assigned hitherto, is probably due to Halley's defence of his chart, contained in *Phil. Trans.* vol. xxix. (Unabridged), 1714. Halley says, p. 165, "to examine the chart I published in the year 1701, for shewing at one View the Variations of the Magnetical Compass, in all those Seas with which the English Navigators are acquainted." But we find that the above number is dedicated "To his Royal Highness, Prince George of Denmark, Lord High Admiral of England, Generalissimo of all Her Majesty's Forces." As Prince George, consort of Queen Anne, did not bear this title until April 17, 1702,² it is evident that the above number is either not the original Halley chart published in 1701, or it is a reprint with a later dedication. If it is to be regarded as an original Halley chart (not a reprint), then a date between 1702 and 1708 must be given it, as Prince George died October 28, 1708. It was published probably not far from 1702, and is in excellent condition.

No. 973 (15). Same title as previous number. Date given in the Catalogue, 1720 (?). I found upon examination that this is identical with No. 974 (5). The Catalogue date is doubtless erroneous. This copy is cut into sections and remounted.

No. S. 112 (6). This is a large folio atlas containing a reprint of No. 974 (5), bearing now the name of the publishing firm, R. Mount and T. Page, and having in addition an extra strip, from 90° to 160° E. of London, pasted on the left-hand side, so that the chart now embraces 430° of longitude instead of 360° as before. The Hamburg and Paris copies are exact duplicates of this, the only difference being that they have pasted below a strip bearing the explanation of the chart by Halley. Prof. Hellmann, in the work cited, has given us the Hamburg text. The Paris text differs in the orthography of a few words, and in the spacing of some of the lines. It appears to be the older text, as below it we find the name of the firm as R. and W. Mount and T. Page, while the name of the firm on the Hamburg text is Thomas Page and William Mount, and the former I have ascertained to have been the earlier firm. This English text I have failed to find attached to the British Museum copies.³

¹ Neudrucke von Schriften und Karten Über Meteorologie und Erdmagnetismus. Herausgegeben von Prof. Dr. G. Hellmann, No. 4. . . . E. Halley, W. Whiston, J. C. Wilcke, A. von Humboldt, C. Hansteen: Die ältesten Karten der Isogonen, Isoklinen, Isodynomen; 1701, 1721, 1768, 1804, 1825, 1826. 4to. 26 pp. 7 plates. (Berlin: A. Ascher and Co., 1895.)

² Kapin de Thoyras's History of England, London, 1751, vol. iii. 1689-1707, p. 544.

³ The atlas contains, besides "An Account of the Methods used to describe Lines on Dr. Halley's Chart of the Terraqueous Globe," &c., by W. Mountaine and J. Dodson, London, 1758, and copies of the Halley chart revised for epochs 1744 and 1756. It bears the title on the back: "Tabulae Nauticae Variationes Magneticas Denotantes. E. Halley." It appears to be a compilation of charts, probably by the authors (Mountaine and Dodson) of the revision.

Nos. 974 (6) and 974 (1) are Dutch editions by R. and I. Ottens, of Amsterdam, of the Halley chart as modified and found under No. S. 112 (6). The base of the chart has been changed, but not the lines of equal variation. The dedication to Prince George has been omitted. The dates assigned by the Catalogue are respectively 1735 (?) and 1740. The chief interest in these Dutch reprints lies in the fact that they have a French text pasted on the left-hand side, and a Dutch text on the right-hand side, over Halley's name.

No. 974 (4). "A new and correct Chart showing the Variations of the Compass in the Western and Southern Oceans, as observed in y^e Year 1700 by his Ma^{ties} Command by Edm. Halley." Date given by the Catalogue, 1720, marked doubtful. This chart extends from 59° N. to 59° S., and from 21½° E. to 100° W. of London. It is enclosed by a border; the base of the chart is entirely different from that of 974 (5); yet the equal variation lines, as far as given, are identical with those for the same region on 974 (5). In but one respect is there a difference in the lines, viz. in no case are they drawn over the land, and in a few cases, also, they are slightly extended. It contains in addition the course of the *Paramour Pink*, the ship in which Halley made his observations, 1697-1700, with the chief aid of which he drew the equal variation lines for the Atlantic Ocean. But the matter of chief importance is that this chart is dedicated to *King William III.* This fixes its date. William III. died March 8, 1702. It is highly probable, then, that this is the chart published in 1701, referred to by Halley in the above quotation, and, in consequence, the *original Halley chart*. It is, moreover, reasonable to suppose that Halley would dedicate his first chart to King William III., who had furnished the means for the making of the observations, to which the chart was due. This chart has escaped the attention of all geomagneticians and bibliographers, and the British Museum copy may be the only one in existence.¹

Another matter of historical interest, apparently unknown to all modern authors in terrestrial magnetism, was ascertained. I find it asserted that the Frenchman, L. I. Duperrey, was the first (1836) to construct the "Magnetic Meridians" for the whole earth, i.e. those lines on the earth's surface marking out the path described by following the direction pointed out by a compass needle. It seems, however, that this honour should be accorded to an Englishman, Thomas Yeates, who, in 1817, published a chart of the Lines of Equal Magnetic Variation, accompanied by a "New and Accurate Delineation of the Magnetic Meridians." A second edition of this chart was published in 1824. Copies of both editions were found in the British Museum.

Washington, April 20.

L. A. BAUER.

The Unit of Heat.

DR. JOLY's strictures on the units of heat at present in use will meet with a ready endorsement from those who have worked on calorimetry. The large calorie is too large for convenience in most cases, and the small calorie is too small, while the confusion created by different writers using different units with the same name is scarcely reduced by their writing one with a capital and the other with a small *c*. A unit of convenient magnitude would be one equivalent to about 100 small calories, and 100 calories has, indeed, been adopted as a unit by more than one writer on thermochemistry. There is, however, what may be termed a natural quantity which is nearly equivalent to such a unit, namely, the heat of fusion on one gram of water at 0° C., which is nearly eighty calories. This appears to be just as suitable from other points of view as the heat of vaporisation of one gram of water at constant temperature and 760 m.m. pressure; and if this latter can be recommended on the ground that in defining it we replace the thermometer by the barometer, the former will possess the superior claim of (for all practical purposes) not depending even on the barometer.

If I remember rightly, this unit has already been adopted in one work on thermochemistry.

No doubt the heat of fusion of water requires redetermination; but it should be determinable with quite as much accuracy as the heat of vaporisation.

Neither of these proposed units, however, possess what should be the chief characteristic of a physical unit, namely, a simple relation to other units; and, before adopting either of them, it

¹ Upon furnishing Prof. Hellmann with a brief description of this chart, he has found that Le Monnier, in his "Loix du Magnétisme," Paris, 1776 and 1778, has reproduced it. Prof. Hellmann's copy of the Halley chart is a duplicate of above, No. S 112 (6), with the exception that it embraces but 360° of longitude. It also has no text.

would be well to consider whether some convenient unit related to, say, the electrical units, could not be adopted. A Committee of the British Association would be a body most suited to investigate this matter.

For practical purposes, a quantity which is even of greater importance than the magnitude of the unit adopted, is the relative value of the heat capacity of water at different temperatures. In spite of the large amount of work which has been expended on this subject, great uncertainty still prevails respecting it. The heat capacity of water, and the heat of fusion of ice, are subjects which I have been for some years intending to turn my attention to, and the work is now practically in hand.

Harpندن, May 4.

SPENCER PICKERING.

My objection to the latent heat of water unit is that this is an inaccessible unit on account of the difficulties attending measurements with the Bunsen calorimeter.

Some years ago I began experiments on a gravimetric ice calorimeter. I have not had leisure to go on with them, but the results obtained were very encouraging. The substance was cooled below 0° while hanging suspended from one arm of a chemical balance. This was effected in a double-walled chamber of copper. A tube, stopped by a plug, connected this chamber with a reservoir of water and clear broken ice. The water was previously boiled to expel air. On raising the plug the water at 0° flows rapidly into the calorimeter, and a shell of clear ice forms upon the substance. The effect on the balance is noted, and by observing the change of buoyancy upon the melting of the ice, and knowing the density of ice at 0°, the mass of the latter can be estimated. The weight measurement will extend to about 0.5 of a calorie. In the steam calorimeter the weight measurement extends to 0.1 calorie, or even less.

There is, of course, much to be said for a thermo-dynamic unit. The question is certainly deserving of having the opinions and views of scientific men fully expressed upon it—as Mr. Pickering suggests. A glance at any of the recent accurate thermal work done in England will show what confusion there exists as to what is the calorie, and as to how all the pet calories of various physicists are related. To render many old measurements of value, this last question should be decided. It reminds one of the state of thermometry in De Saussure's time.

Trinity College, Dublin.

J. JOLY.

Reputed Traces of Negrito Pygmies in India.

MAY I be permitted to suggest to readers of M. Quatrefages' work on the Pygmies, the English edition of which has recently been reviewed in NATURE, to pause before accepting his conclusions as to traces of Negritos being found in peninsular India.

The evidence he relies on partly consists of a description by M. Rousselet of a half-starved wanderer from Sirjuga, whom he assigns to the race Bander Lokh (or, as it is printed in the English edition, Bander Lokh) and the tribe Djangal. Any Anglo-Indian with the slightest knowledge of the language, not to say of ethnology, would be amused at such nicknames being applied as definite racial terms. The first simply means monkey-people (equivalent to savage), as applied by dwellers in the plains to the wilder inhabitants generally; and the second, if it can be said to mean anything in the form presented, is simply "jangli," or a dweller in jungle.

The portrait of this "Djangal," from a rapid pen and ink sketch, is a caricature of a somewhat exceptional and by no means typical individual, and affords no trustworthy material for an ethnological discussion.

The "fever-stricken inaccessible" region Sirjuga,¹ from whence this specimen was a fugitive, according to M. Rousselet, is well known to me, and when travelling there I spent some days in the company of the late General Dalton; and not only then, but in connection with the production of his great work on the Ethnology of Bengal, to which I had the privilege of contributing, I had many conversations with him regarding the tribes of that region. I was, moreover, well acquainted with the true Negritos of the Andamans, of whom I had then already seen many; and I do not hesitate to say that I never met with the slightest trace of a Negrito element among the numerous tribes I became acquainted with during many years travelling in the hilly tracts of Western Bengal, the Central Provinces and the Northern Provinces of Madras. Individuals belonging to different tribes

¹ The district of Sirjuga in Chota Nagpur is not near Amerkantak, nor is it included in the Vindhyan Range as is stated by M. Quatrefages.